make and break of the battery current as the test of excitability. They find that the effect of the make excitation is increased when it falls upon a katelectrotonic region, polar or peri-polar, that the effect of the break excitation is diminished when it occurs in an anelectrotonic region, polar or peri-polar, and that increase and diminution are more marked in the case of the polar than that of the peri-polar region.

They also tested the polar region by mechanical excitation, and obtained evidence of increased excitability at the kathode, of diminished excitability at the anode.

The authors have also observed "after-effects" of polarisation corresponding with the after-effects of electrotonus in the frog's nerve as described by Pflüger.

The experiments were for the most part made on the peroneal nerve, which was selected on account of its superficial course, and the facility with which the muscular responses could be recorded graphically.

IV. "On the Excretion of Nitrogen by the Skin." By J. Byrne Power, L.C.P.I. Communicated by Professor Emerson Reynolds, F.R.S. Received February 7, 1882.

During the years 1877–78, I conducted a series of experiments on the excretion of nitrogen by the skin. Some of the data then obtained were communicated at the Dublin Meeting of the British Association, but I have since extended the inquiry, and now beg to submit an account of the investigations.

The results obtained by various experiments as to the existence of nitrogen in the sweat have been contradictory. Voit,* Ranke,* Parkes,† and others, relying on indirect methods, have denied its existence, finding that the quantity excreted by the kidneys and intestinal tract was equal to, and in some cases even exceeded, that ingested, therefore leaving no room for any excretion by the skin.

On the other hand, Anselmius,‡ Berzelius,§ Favre,|| Funke,¶ and

* "Schmidt's Jahrb.," Bd. cxvii, pp. 1—10. Voit made further experiments on doves with confirmatory results. On the other hand, Seegen and Nowak made subsequent experiments upon dogs with opposite results; these again are contravented by Gruber ("Virchow and Hirsch, Jahrgang," Bd. I, 1881, p. 163). I do not myself believe that experiments on the lower animals are conclusive on this point in human physiology.

† "The Lancet," 1871, vol. i, p. 400.

‡ Berzelius, "Traité de Chimie." Traduit par M. Esslenger. Tom. vii, p. 324. Paris: 1833.

§ *Op. cit.*, p. 325.

|| "Archiv Gén. de Méd.," 1853, Tom. ii, pp. 1—20.

¶ "Beiträge zur Kenntniss der Schweissecretion." Moleschott's "Untersuchungen zur Naturlehre," iv, 36.

others who have analysed portions of the sweat collected by different methods have found nitrogen, though Schottin and Ranke failed to do so. Funke, by his experiments upon himself and his two pupils, published in 1858, not only found nitrogen in the sweat, but also proved its existence as urea, and was the first to make an estimation of the quantity excreted by the skin of the entire body in a given time. Adopting Schottin's method, he collected, by means of a caoutchouc sleeve, the sweat excreted by the arm in a given time; and, having filtered, he estimated the quantity of nitrogen present by the combustion process. Then, having established by measurement a ratio between the superficies of the arm and that of the entire body, he calculated the entire cutaneous excretion. Dr. Fleming,* adopting the same method, arrived at almost identical results. I may add that Dr. Fleming's experiments were made subsequently to mine.

In addition to the unsatisfactory character of all indirect proofs, the investigations of Voit, Ranke,† Parkes, and others seem liable to a further objection, arising from the extreme difficulty of ascertaining the exact quantity of nitrogen ingested. Again, as already noted by Parkes, the amount of nitrogen excreted, due to waste of animal tissue during the experiment, cannot be determined. These objections seemed to me to make such negative results of doubtful value upon this point; and Professor Parkes' conclusion that, "apart from detached skin structures the balance of evidence is against the passage of nitrogenous substances by the human skin," to my mind, at least, required confirmation. As regards the direct evidence of nitrogen in the sweat the researches of Funke, confirmatory of those of Anselmius, Berzelius, Favre, and others seemed to me to be conclusive on the point.

Funke's method is the only one by means of which an estimation of the entire cutaneous excretion in a given time has been made, and it alone required re-examination. Funke's method seemed to me to be open to the following amongst other objections: first, the uncertainty attaching to the relative measurement of the superficies of the arm and that of the entire body; secondly, that arising from the two underlying assumptions—(*a*) assumed equality of secretive power of rest of body to that of arm, (*b*) assumed identity of chemical composition of sweat from the arm with that excreted from the rest of body. Considering these grave objections to Funke's method, and the general uncertainty as to any nitrogen being excreted by the skin, I determined, if possible, to ascertain the quantity of nitrogen excreted by the entire skin in a given time without reference to measurement or actual amount of fluid sweat excreted.

* "Journ. Anat. and Physiol.," vol. xiii, p. 454.

† Ranke, as above alluded to, endeavoured to obtain nitrogen directly by analysis of the sweat, and failed to do so, but seems to have mainly relied on the indirect argument.

I shall now briefly state the mode of collecting the excretion which I adopted. Having first tested the atmosphere of the ward in which I was about to operate, to ascertain that it did not contain free ammonia in any quantity, I placed upon one of my hospital beds an india-rubber sheet, and over it another sheet of pure linen, upon which the person experimented upon lay. Over his body was placed a wooden cradle or canopy, covered outside with a thick felt, and lined inside with linen, which coverings were to be carefully adjusted round his neck. To raise the temperature within the canopy I used the lamp-furnace invented by the late Surgeon-Major Wyatt, the flue from which fitted accurately through a hole in the coverings guarded by a wooden ring. Considering the spirit lamp of Wyatt's furnace objectionable for many reasons, I substituted a Bunsen gas-burner. To insure the regular renewal of the air within the canopy, and to prevent its saturation, as well as to collect any free ammonia which might be evolved, I introduced a tube leading from a Bunsen air-pump, which tube was connected with two glass towers filled with large glass beads, and charged with half an ounce of dilute hydrochloric acid of known strength. Through another hole in the canopy I introduced a hydrometer, by which I was enabled to observe the temperature, and calculate the degree of saturation of the atmosphere within. As the gas, water, and reagents employed contained some small portion of nitrogen, my first task was to ascertain the constant error arising from this source. To effect this I performed three blank experiments, omitting only the introduction of the person to be experimented upon. The result was that I obtained a small quantity of nitrogen, nearly equal in each case, viz., 0·066, 0·066, and 0·061, and having a mean value of 0·064 grm., that being the total amount collected in one hour under the experimental conditions. The water I used was Vartry water, it being the water supply to my hospital, and when I used it unfiltered I employed the above number as a constant. As it would be necessary when I wished to get rid of epithelium to filter the portion of the bath water I took for analysis, I made three similar blank experiments, using filtered Vartry water, and obtained another constant, amounting to 0·0408 grm. of nitrogen, which I used in all such experiments. The method employed for estimating the nitrogen will be described later on. In the nine experiments in which I estimated the chlorides I took the precaution of finding the amounts of chlorides present in the Vartry water on that day, as I found the quantity of chlorides present in it liable to periodic variations.

I now commenced a series of experiments upon myself, with the assistance of my clinical clerk, Mr. Clune. One of these I shall describe:—I first took a sponge bath for the purpose of removing loose epithelial scales, as well as minute fibres from the underclothing,

which I always found adhering to the skin, and having noted my pulse, respiration, bodily weight, and temperature, I entered under the canopy.* The coverings being carefully adjusted round my neck, the gas furnace was lighted, the air-pump and hydrometer adjusted, and the experiment continued for an hour, the time being carefully noted. Before leaving the canopy the pulse respiration and bodily temperature were again noted, also the mean temperature and point of saturation of the atmosphere within the canopy. On leaving the canopy I got into a bath containing twenty litres of Vartry water, acidulated with half an ounce of dilute hydrochloric acid of known strength. I took with me into the bath the linen sheet upon which I had lain whilst under the canopy, and with it I gently rubbed myself so as to remove any loose epithelial scales. On leaving the bath I again weighed myself, and in twenty minutes after I again noted my pulse, respiration, and bodily temperature. I caused the linen and india-rubber sheets, as well as the towers containing the dilute hydrochloric acid, to be washed in the water of the bath, and then brought a specimen of it to my laboratory for analysis. On two occasions I analysed the contents of the towers separately and got hardly a trace of ammonia with Nessler's test, proving, in these instances, at least, that free ammonia was not given off by the skin. The process of analysis of the water which I employed was briefly as follows:—I carefully measured 100 cub. centims. and poured it into a fractional distillation flask, which I altered to suit my purpose by shortening and bending the side tube upwards towards the mouth. I now took a porcelain dish, in which I placed a small quantity of pure sand, carefully cleansed by hydrochloric acid and subsequent washing with distilled water, and moistened it with a drop of strong, ammonia-free, sulphuric acid. Having placed the dish upon a water-bath, I inverted the flask into it, and, properly suspending it, carefully evaporated the water as it gradually flowed into the dish. When nearly dry I removed the sand from the dish and mixed it with soda-lime, in a small combustion tube, and proceeded to estimate the quantity of nitrogen contained in the residue in the manner referred to in a former paper.† Hence I calculated the quantity contained in the twenty litres, and from this I deducted my constant, thus ascertaining the total quantity of nitrogen excreted by the skin during the experiment. I have described an experiment in detail, as all the

* In my communication to the British Association, I gave the results of my observations on the effect of the hot air bath upon the rate of the pulse and respiration and on bodily temperature. I have since seen papers by Dr. Fleming, *op. cit.*, and Dr. C. Large ("Archiv Gén. de Méd.," Tom. i, 1880, p. 150), on the physiology of the Turkish bath, which go more fully into the immediate effects of artificially increased temperature upon the human body than I did, and I find that their results are confirmatory of my own.

† "Dublin Journal of Medical Science," vol. lix, No. 38, p. 81.

others were conducted in a similar manner. But in some instances I also filtered the aliquot part of the bath-water in order to get rid of epithelium, and then estimated the nitrogen in 100 cub. centims. of the filtrate; the result gave me the soluble nitrogen, *i.e.*, nitrogen present in some soluble compound, and the difference between this value and that afforded by the fluid containing suspended epithelium gave me the weight of the nitrogen present in the insoluble condition in the same portion of the bath-water. In the tabular statement I have noted the instances in which this additional estimation was made. I may here note that I examined the deposit from the water of the bath under the microscope, and found it to contain scarcely a trace of anything but epithelial scales.

Funke, by the only two completed analyses which he made, found in one case 0·198 grm. and in another 0·2935 grm. of soluble nitrogen excreted by the entire skin in one hour. But it must be remembered, as I have already pointed out, that his numbers are calculated values from imperfect data, and are not the results of direct determinations of the quantities excreted by the entire body in a given time.

In the accompanying table I give the results of twenty-five experiments upon six different individuals, viz., two healthy subjects and four hospital patients: cases which I considered suitable for treatment by the hot air bath. It will be seen that the greatest quantity of soluble nitrogen which I find to be excreted by the entire skin, in a case of Bright's disease (B) is 0·2392 grm. per hour. In experiment 1, case A (a healthy subject) we find that the quantity of soluble nitrogen excreted per hour is as low as 0·038 grm. And I find that the mean result of all my direct experiments, upon healthy and unhealthy subjects, gives 0·0824 grm. of soluble nitrogen collected from the entire skin in one hour. The difference between Funke's and my results may be in part due to the circumstance that my experiments were made upon the body in a state of rest, while Funke's were made under conditions of violent exercise, which I, accepting the views of Liebig, so strongly supported by the more recent experiments of Professor Flint,* believe to be always accompanied by waste of animal tissue and consequent increased excretion of nitrogen. With regard to Funke's estimation of the *possible* excretion of nitrogen per diem, amounting to as much as from 4·76 to 7·045 grms., I believe it to be excessive. He arrives at these results by multiplying the quantity obtained in an hour by twenty-four, necessarily assuming the constancy of the sweat secretion, which assumption is contradictory to the statement made in another part of his paper, to the effect that "in one or two hours the quantity of the supply begins to diminish, even though the temperature and movement remain un-

* "American Journ. Med. Science," vol. lxiii, n. s., p. 163.

altered, and falls to such a minimum that one can scarcely perceive the increase even during greater intervals of time."

I believe that under ordinary circumstances the excretion of nitrogen by the skin is very small indeed, even in cases of gout (Case D) and Bright's disease (Case B), in which I expected to find it present in larger amount. This being so I can now well understand how it was that Voit, Ranke, and Parkes, by their indirect methods came to the conclusion that there was *no* such excretion, yet I do not deny that under extraordinary circumstances, such as those mentioned by Leube,* Deininger,† Kaup and Jurgensen,‡ Taylor,§ Schottin,‖ and others, crystals of urea may have been found upon the skin. However, with the exception of the case of Leube there does not appear sufficient evidence that adequate care was taken to ascertain that the crystals found were really those of urea and not of other salts, such as sodium chloride, crystals of which, as is well known, frequently simulate those of other bodies. I find that Leube notices this as an objection to the researches of former experimenters, and to obviate it he treated the mass removed from the skin with baryta-water and absolute alcohol in the usual manner, and so undoubtedly proved the presence of urea. Dr. Taylor seems also to have proved the presence of urea in the "saline mass" removed from the skin in a case of uræmic poisoning. Owing, I presume, to the smallness of the quantities obtained, none of the above-named experimenters give the quantity of the saline matter, nor the amount of urea found in it, with the exception of Kaup and Jurgensen, who state that they obtained 8·4 grms. from the shirt worn by a choleraic patient. My method of collecting the cutaneous excretion afforded me an easy means of ascertaining the quantity of sodium chloride excreted, and in the table will be found the quantities obtained in nine experiments. In five of these the amount excreted can be compared with that of soluble nitrogen, and it will be seen that the quantity of sodium chloride is comparatively great, the proportion to nitrogen, which is nearly constant, being as 10 : 1. In conclusion I would observe that though I found nitrogen to be excreted by the skin in all cases, yet the quantities are so small that I do not believe the cutaneous excretion can ever act vicariously towards the renal to any appreciable extent. I now submit this paper to the Society in the hope that some one having more leisure and more ample resources at his command may further prosecute the inquiry.

* "Deutsches Archiv für Klinische Medicin," Bd. vii, p. 1.

† *Id.*, Bd. vii, p. 587.

‡ *Id.*, Bd. vi, p. 55.

§ *Op. cit.*

‖ Schottin, though he failed to find nitrogen in normal sweat as already mentioned, succeeded afterwards in doing so in a case of renal disease ("Schmidt's Jahrb.," Bd. 74, s. 9).

Reference to Case.	Remarks.	Number of experiment.	Period of year.	Duration of experiment.	Average temperature of bath.	Weight of body.	Age.	Average renal excretion of nitrogen during experimental period.	Cutaneous excretion.			
									Total nitrogen.	Soluble nitrogen. *	Insoluble nitrogen. †	Chlorine in terms of NaCl.
				h. m.	0° F.	Lbs.		Grms.				
A	Healthy subject	1	July	1 0	Not accurately observed, about 120°.	124	40	11·93	0·048	0·039	0·009	
		2	„	1 0		..	..	..	0·112			
		3	„	1 30		..	..	..	0·128			
		4	„	1 0		..	..	..	0·096			
		5	„	1 0		..	..	..	0·132			
		6	„	1 0		..	..	..	0·128	0·091	0·037	
	a. Experiments 6 and 7, made on the same day	7	„ *a*	0 45		..	..	..	0·080			
		8	„	1 0		..	..	..	0·096	0·055	0·041	
	b. Experiments 8 and 9, made on the same day	9	„ *b*	1 0		..	..	..	0·128	0·071	0·058	
B	Bright's disease, chronic, dropsical, sweating easily produced.	1	Aug.	1 0	124	155	43	..	0·336	0·2392	0·0968	
		2	„	1 0	127	..	..	..	0·336	0·1792	0·4568	
		3	„	1 0	118	..	..	..	0·116	0·052	0·064	
		4	„	1 0	123	..	..	..	0·256	0·1192	0·1368	
C	Catarrh, urine loaded with lithates, otherwise healthy.	1	Jan.	1 10	131	161	21	19·3	0·128	0·039	0·089	
		2	„	1 10	142	..	..	..	0·160			
		3	„	1 0	125	..	..	..	0·176			
D	Gout, convalescent	1	Nov.	1 0	139	142	60	..	0·160	0·1292	0·0308	1·25
		2	„	1 0	146	..	..	..	0·262	0·1672	0·0948	1·65
F	Sub-acute rheumatism, sweating difficult to produce.	1	Jan.	1 30	135	110	16	..	0·072	..	..	0·33
		2	„	1 10	139	..	..	..	0·056	..	..	0·52
		3	„	1 15	139	..	..	..	0·106	..	..	0·39
		4	„	1 30	143	..	..	..	0·075	0·0472	0·0278	0·52
		5	„	1 15	149	..	..	..	0·086	..	..	0·62
J	Acute nephritis	1	Dec.	1 15	120	162	40	6·92§	0·1425	0·125	0·017	1·26
		2	„	1 25	135	..	..	..	0·125	0·096	0·029	1·00

* *i.e.*, nitrogen present in some soluble compound, *e.g.*, urea, &c. † *i.e.*, nitrogen present in some insoluble compound, *e.g.*, epithelium.
§ Less nitrogen as albumen.